REPLIES

BY

PROF. LAWRENCE S. BENSON,

Author of BENSON'S GEOMETRY,

TO

PROF. E. T. QUIMBY,

Dartmouth College, Hanover, N. H,

PROF. WM. CHAUVENET, LL.D.,

Washington University, St. Louis, Mo,

PROF. ROBERT D. ALLEN,

Kentucky Military Institute, Farmdale, Ky.,

AND

PROF. A. T. BLEDSOE, LL.D.,

Formerly University of Virginia, now Editor Southern Review, Baltimore, Md.

NEW YORK:
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CRITICAL EXAMINATIONS

OF

BENSON'S GEOMETRY.

Before entering upon the subject matter of this treatise, I will make some preliminary remarks. About ten years ago, I published my "SCIENTIFIC DISQUISITIONS CONCERNING THE CIRCLE AND ELLIPSE," in which I advanced new views on the circle, and demonstrated that the circle is the *arithmetical mean* between the circumscribed and inscribed squares. I also offered a reward of ONE THOUSAND DOLLARS to any one who refuted the demonstration. The reward is still standing, although there were many claimants for it. One, by the *nomme de plume* of "DALTON," controverted my conclusion, and, in consequence, a lively and interesting correspondence ensued in some prominent Southern journals, when, finally, two committees of expert mathematicians were appointed to decide upon the issues involved. These committees were: Major P. F. STEVENS, Sup't of the South Carolina Military Academy, Prof. CHARLES S. VENABLE, now of the University of Virginia, and WILLIAM B. CARLISLE, Esq., editor of the Charleston Courier, for myself: and Prof. LEWIS R. GIBBES, of the Charleston College, Prof. JAMES H. CARLISLE, of Wofford College, and Rev. J. S. KIDNEY, of St. Thaddeus' Academy, Society Hill, South Carolina, for my respondent.

No decision was ever arrived at by these committees, and in March, 1864, I went to Europe to lay my views before prominent mathematicians and the various scientific and philosophical societies there; but I did not obtain any decision from them.

Soon after the late civil war, I came to New York, and proposed to remodel the *Elements of* EUCLID so as to substitute *direct* demonstrations for the *Reductio ad Absurdum.* This plan received the approval of Prof. G. B. DOCHARTY, LL. D., of the College of the City of New York, Hon. S. S. RANDALL, City Superintendent of the Board of Education of New York, Prof. J. G. FOX, Principal of the Free Schools of Cooper Union, NEW YORK ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AND ART, SCIENTIFIC AMERICAN, NEW YORK TRIBUNE, and many mathematicians and scientific journals in all parts of the country.

In giving the direct demonstrations in all cases, I had to consider the *Reductio ad Absurdum* of Euclid, whereby he endeavored to sustain the proposition "that the area of the circle is the rectangle on the semi-circumference and radius." See Benson's Geometry, pp. 139, 140, 141, 142, 143. Now, if this proposition be true, it can be *directly* demonstrated, which all true propositions can be; and the fact that it is impossible of being directly demonstrated, is a positive proof that it is not true. And, in giving the direct demonstration for the problem of the circle, I was unavoidably led to a different result from the false proposition of Euclid.

I will give a sketch of my first demonstration, which is totally different in *method* from those I published in Benson's Geometry; but they all give the same conclusion.

It is well known to all students of Geometry that Hippocrates, of Chios, was the first geometer who quadrated any portion of the circle; he effected the quadrature of the lune—the portion of a circle, in the form of a crescent, contained by the semi-circumference of one circle and the arc of the quadrant of another.

(See Diagram to the Twenty-fourth Proposition of the Fifth Book, Benson's Geometry.) A D C is the lune, equivalent to the triangle A C B; hence, we have the segment of the quadrant, A C, unquadrated; and since a similar construction can be made in the other half of the circle, A D C B, we have the entire circle quadrated with the exception of the circular space in the middle composed by two segments of quadrants; consequently, to effect the quadrature of the circle, we have simply to quadrate the segment of a quadrant.

In view of this, I considered the nature of *the segment of a quadrant:* I saw that its properties were the *arc, chord* and *altitude.* Knowing that geometrical quantities are *abstract* things, and that Geometry is entirely a *hypothetical* science, I reasoned from the hypothesis *that the arc of the segment of a quadrant is* geometrically *equivalent in length to the chord and altitude of the segment:* which makes the *arc* equivalent to the side of the *mean square* between the circumscribed and inscribed squares; and the circle equivalent to three times square of radius.

These are the outlines of my first demonstration. The dissenters to it, without reasoning upon the course of argument, objected at once to the conclusion.

To disprove which, "Dalton" cited the case of an inscribed *dodecagon* being three times square of radius, and added that it is impossible for a part to be equal to a whole.

My reply was, that in reasoning about the circle, we must bear in mind the difference between *it* and a *polygon;* that where one is formed by a *curve*, the other is formed by a certain number of *straight lines;* and that, consequently, the circle can not be treated as a

polygon ; hence, conclusions drawn from the nature of the curve must be incongruous with conclusions drawn from the nature of the straight line. In proof of my conclusion I showed that it belonged to the class of truths derived from the nature of the circle, amongst which I embraced the truths discovered by ARCHIMEDES, relating to the cone, sphere and cylinder, their convex surfaces, and the quadrature of the lune ; with all of these my conclusion agrees (See BENSON'S GEOMETRY, pp. 166, 216, 217,)—an irrefragable proof that my conclusion is true; for it is the chief characteristic of geometrical truths that they all agree with one another, however they may be combined together, provided that the reasoning is correct.

Now, "DALTON" and the other dissenters overlooked the fact of the above agreements, and they fell into the blunder that logicians call *fallacia accidentis*. Certainly, when a conclusion agrees with *numerous* truths, that evidence of its truth can not be impaired because that conclusion is incongruous with *another* conclusion derived from a totally different source.

Another dissenter (J. H. SCHULTZE) had two tin cups constructed: one, round, $3\frac{1}{2}$ inches diameter, and 3 inches deep; the other, square, 12 inches perimeter, and 3 inches deep—inside measurements; they held same quantity of water.

My demonstration makes a square of 12.08+ inches perimeter equivalent to circle with $3\frac{1}{2}$ inches diameter. By the method of EUCLID, we have perimeter of polygon equivalent to circumference, $3\frac{1}{2} \times 3.1415926+ = 10.9955741+$, showing that in the matter of the perimeter my method is more practically correct than the method of EUCLID. Again: a square of 12 inches perimeter is equivalent to 9 square inches. My demonstration gives 9.1875 square inches, exactly, for the square equal to the circle, whereas the method of EUCLID gives 9.6211+ square inches for the same square; hence, it is clearly seen that my demonstration is more *practically* correct than the method of EUCLID; and had the mechanical construction of the tin cups been more perfect, the difference between it and my demonstration would be imperceptible. When I published these figures, they were a quietus to Mr. SCHULTZE; so that, scientifically and practically, I sustained my demonstration.

I will now give the criticisms which have been made to the demonstrations published in BENSON'S GEOMETRY. Prof. E. T. QUIMBY, Dartmouth College, Hanover, New Hampshire, has written me about thirty letters; and although they are eminently controversal, I have failed to discover one ill-tempered sentence, and his whole correspondence bears evidence of a desire to investigate for truth's sake. He directed his attention first to the matter of the excess that I claim the

METHOD OF APPROXIMATION gives for polygons inscribed in the parabola. He writes: "I have been looking at your approximation computations of the area of the parabola in which you get an excess above the $\frac{2}{3}$ rectangle. I have not made the numerical computations, because I presume you have been over them times enough to secure accuracy. If you have made no mistake of this kind, I think the difficulty must be in not carrying out your work to a sufficient number of decimals. Carry it out to 15 decimals, and see if your excess is not less."

In reply, I showed by examples that the sum obtained by adding together a series of decimals is greater when we have a greater number of decimal places; and that, had I used fifteen decimals in my computations, I would have obtained a greater excess than I obtained by using six decimal places only.

The METHOD OF APPROXIMATION is the only method geometers use to obtain the area of the circle, and this method gives 3.1415926, &c., square inches for the area of the circle, when the radius is unity.

And since the area of the parabola has been geometrically determined by ARCHIMEDES, therefore, when the METHOD OF APPROXIMATION gives an *excess* above the area of the parabola for the area of *inscribed* polygons, most evidently THAT METHOD IS UNRELIABLE FOR DETERMINING THE AREAS OF THE CIRCLE AND OTHER CURVILINEAR SPACES.

Prof. QUIMBY next directed his attention to Corollary 2, Proposition 17, Book 6, BENSON'S GEOMETRY. He wrote: "In this corollary I see no flaw till you say, as an inference from the fact that the triangle B S N and the segment B N generate equivalent solids, 'consequently the segment B N and the triangle B S N are equivalent.' Now, this inference is not legitimate; and not only does it *not* follow that the segment and triangle are equivalent, but it does follow that they are not equivalent; for, since they generate equivalent solids, it must be true that the part of the segment not belonging to the triangle generates a solid equivalent to that generated by the part of the triangle not belonging to the segment. Now, since these two parts, viz., segment B T and area T S N, generate equivalent solids, the area of one, multiplied by the distance its center of gravity moves—that is, by the circumference of the circle described by its center of gravity—is equal to the area of the other multiplied by the circumference described by its center of gravity. But the center of gravity of T S N evidently describes a larger circumference than that of B T. Hence, the area of B T is greater than T S N. Hence, the area of the segment B N is more than the triangle B S N."

In reply, I requested Prof. QUIMBY to show me by what principle of reasoning or method of demonstration he could sustain the proposition that *the volume of a body of revolution is equivalent to the generating*

surface multiplied by the circumference of the circle described by the center of gravity of the generating surface.

Prof. QUIMBY wrote: "Will you admit this proposition? The volume generated by any plane figure revolved about a line in its own plane is measured by the area of the figure revolved multiplied by the mean circumference described by its points. I mean by this, that the area of the revolved surface is to be multiplied by the *average* distance its points travel. If that distance is not the same distance its center of gravity moves, then *it is* the distance some other one of its points moves. I will state the proposition to correspond to this last idea: The volume generated by a plane figure revolved (as above) is measured by the area of the figure multiplied by some one point of the figure. Hence, to apply it to our revolved surfaces: Vol. B T = area B T $\times$ circ. of some point of B T; vol. T S N = area T S N $\times$ circ. of some point of T S N; $\therefore$ area B T $\times$ circ. of some point of B T = area T S N $\times$ circ. of some point of T S N. But circumference described by any point of B T must be less than that described by any point of T S N. $\therefore$ area B T $>$ area T S N."

My reply was that Prof. QUIMBY had entirely overlooked the point I urged, which was to know by what course of reasoning he could prove that a volume of revolution is the product of the generating surface and *the circumference described by any point of the surface.*

Prof. QUIMBY, in lieu of demonstration, stated that it was universally acknowledged by all mathematicians that "the solid generated by the revolution of a plane figure about an axis in its own plane is measured by the area of the figure into the circumference described by its center of gravity."

Surely, then, if this proposition be so *universally* acknowledged, its proof should be possible; and I thought it strange that Prof. QUIMBY never gave it, when I asked him for it; and I was ready to admit all his reasoning, when he proved it to me. If Prof. QUIMBY had no way to prove it, he had no right to use it, and his reasoning is untenable.

I am perfectly aware that mathematicians universally acknowledge that proposition; at the same time, I am aware that their argument for it is based upon the *Reductio ad Absurdum* reasoning; and *if the proposition be true,* IT CAN BE DEMONSTRATED ALTOGETHER DIRECTLY, which can not be done: hence, *prima facie,* the proposition is false.

Prof. QUIMBY took another tack. He asked my assent to the following proposition, which I admitted: "When two equivalent surfaces are moved in a line perpendicular to their plane, or revolved, the one which moves farther generates the greater volume." Prof. QUIMBY then wrote: "Let us look at your proof again. It is based on this, in your own words (BENSON'S GEOMETRY, p. 165,): 'When we have equivalent solids generated upon the same radius, the generating sur-

faces are equivalent.' I have said this is not true; but I will retract that statement so far as to say that its truth or falsity will depend upon the meaning you attach to the words 'upon the same radius.' With the meaning you evidently give those words, the statement is not true. That is to say, in your diagram in p. 164, the segment B T and the area T S N are not 'upon the same radius' in any sense that can make the statement true; and yet your demonstration requires you to say that those areas are equivalent because they generate equivalent solids. Is it not so? Now I claim:

"1. You must make segment B T equivalent to T S N, to prove area of circle equal to 3 R^2.

"2. *You have shown* B T and T S N to generate equivalent solids.

"3. Since *every point* of T S N is farther from the axis of revolution than *any point* of B T, T S N must be less than B T.

"4. Hence, area of circle *is not* equivalent to three times square on the radius, but is more than 3 R^2.

"I am unable to conceive what you mean by saying that B T and T S N are on the same radius, and I am equally unable to conceive how you can dispute my statements above and be sane on mathematical questions."

The difficulty here with Prof. QUIMBY was about what I meant by the words "upon the same radius." I wrote him that I meant *the radius of computation* (See BENSON'S GEOMETRY, p. 217.) I meant the radius by which the volumes generated by B T and T S N are computed. I asked him to compute those volumes without using the *same* radius. He could not do so, hence the volumes have THE SAME RADIUS OF COMPUTATION; hence, however unequally their positions may be from the axis of revolution, B T AND T S N ARE CORRESPONDING PARTS OF VOLUMES GENERATED BY THE SAME CONDITIONS, that is, in the *same* plane, around the *same* axis, with the *same* radius: having the *same* base, the *same* altitude, and the *same* solidities. We obtain the contents of the volume generated by the segment B T: first, by finding the contents of the cylinder generated by the rectangle P N E B; secondly, taking two-thirds of these contents, we have the volume of the hemisphere, of which the volume generated by B T is a part; thirdly, we find the contents of the frustum of the hemisphere made by a plane through the point T parallel to the base P N; fourthly, we find the contents of the cone generated by the triangle formed by the above plane, the line T B, and the axis of revolution; and lastly, we have the contents of the volume generated by B T, by taking the sum of the frustum and cone from the hemisphere. We obtain the contents of the volume generated by T S N, by taking the sum of the same frustum and cone from the volume generated b P N S B, which solid is computed by means of the radius of the base

of the cylinder generated by P N E B: hence, in all these computations, we have THE SAME RADIUS AS THE BASIS OF THE CALCULATIONS. Therefore, the volumes generated by B T and T S N are computed under the *same* conditions; and since their *computations* are dependent upon their *generation*, they are, therefore, *generated* under the *same* conditions. Which must necessarily be, because these volumes are corresponding parts of the volumes generated by P N B and P N S B, which are generated by the *same* conditions. Consequently, THE EQUIVALENCE BETWEEN P N B AND P N S B IS NOT IMPAIRED BY THE POSITION OF B T AND T S N.

Prof. QUIMBY, after this, undertook to defend the *Reductio ad Absurdum*. He reasoned thus: "You reject the *Reductio ad Absurdum*, saying that it does not amount to proof. I think it does. I say nothing now of any particular theorem, but of the method in general. Suppose I do this. There are two magnitudes, A and B; I show, by strict mathematical demonstration, that the assumption that A is greater than B leads to absurdity; I show, in the same manner, that to assume A less than B leads to a similar absurdity: Have I not shown, then, that A can not be either greater or less than B, and therefore must be equal to B? If to prove that A can not be anything else than B, is not proving that A is B, I don't know anything about demonstration. I freely admit that the direct demonstration of a theorem is usually preferable; but I do not admit that the indirect is not logical. So much for the *Reductio ad Absurdum*."

Had Prof. QUIMBY, without *supposing*, but "BY STRICT MATHEMATICAL DEMONSTRATION" *shown*, that A can not be either greater or less than B, most undoubtedly he would have proven A equal to B. He would, in *that* case, have given a very *direct* demonstration, and would have *himself* repudiated the *Reductio ad Absurdum*.

Now, taking the method of the *Reductio ad Absurdum* "in general:" It is reasoning from a supposition which we *know* to be false; hence, as its name implies, it leads to absurdity, which can not be otherwise, since it is undoubtedly *absurd* throughout, from *beginning* to *end*.

Now, I admit that when we have two magnitudes, A and B, A is either greater than, less than, or equal to B; and if A be shown to be neither greater nor less than B, A is then equal to B.

In the *Reductio ad Absurdum*, it is supposed, in the first place, that B is greater than A, and, for "*the sake of argument*," (?) B is assumed to be equal to a third magnitude, C, which is *known* to be greater than B; hence, the conclusion is absurd; and the inference is that because A can not be equal to C, A can not be equal to any magnitude that exceeds B; but no proof is given why A can not be equal to a magnitude *less than* C and *greater than* B, for it is not shown that exceeds B by less than any assignable magnitude.

In the second place, in the *Reductio ad Absurdum*, it is supposed that B is less than A, and, for "*the sake of argument*," (?) B is assumed to be equal to a fourth magnitude, D, which is *known* to be less than B; hence, the conclusion is again absurd, and the inference is that because A can not be equal to D, A can not be equal to any magnitude less than B; but no proof is given why A cannot be equal to a magnitude greater than D and less than B, for it is not shown that B exceeds D by less than any assignable magnitude.

Because A and B are both less than C, and are both greater than D, it does not necessarily follow that A and B are equal, for it is possible for A and B to be both less than C, and to be both greater than D, and still be unequal to one another. Let C be 9, and D be 6, A can be 8, and B can be 7. Now, A and B are both less than C, and are both greater than D, *still there is no equality between A and B.* So that the *Reductio ad Absurdum* is not only inconclusive in demonstration, but it is also illogical in its *modus operandi.*

The sophistry of this reasoning is in supposing that A can not be either greater or less than B, because A and B are *both* less than C and are *both* greater than D; but no proof is given to show that C exceeds B by some assignable magnitude equal to the excess of C over A, and no proof that B exceeds D by some assignable magnitude equal to the excess of A over D. It is surprising that the insufficiency of the *Reductio ad Absurdum* should have been overlooked, although it is on record that some of the ablest mathematicians have objected to the *Reductio ad Absurdum.*

In regard to the proposition that A is either greater than, less than, or equal to B, it will be more in accordance with "STRICT MATHEMATICAL DEMONSTRATION" to prove directly that A is equal to B, than to use false premises, circuitous processes and sophistical arguments which finally result in absurd conclusions. For, if A be equal to B, it can be proven so by *direct* demonstration; and if A can not be proven by *direct* demonstration equal to B, it is evidently clear that A and B are unequal.

In Logic, there is the *Negative Reasoning*, which is perfectly valid, and it is a very useful instrument of investigation; but it differs materially from the *Reductio ad Absurdum*, since it has no false premiss, no circuitous process, no sophistical argument, and no absurd conclusion.

Prof. QUIMBY again wrote: "Only one thing more: I want to ask you why, in revolving this parabola and triangle, I do not prove, as you prove your area of the circle, that the parabola is $\frac{5}{8}$ of the rectangle of its ordinate and abscissa." [Here Prof. QUIMBY drew a diagram of the semi-curve of a parabola, circumscribed by a rectangle.] "Let the curve A C be a portion of a parabola, of which

A D is the axis. Revolved about A D, it generates a paraboloid which is $\frac{1}{2}$ of the cylinder generated by the rectangle B D. Take E C, $\frac{1}{4}$ of B C. Draw E A. The trapezoid A E C D generates a solid $\frac{1}{2}$ of the *same* cylinder, therefore the section of the parabola and the trapezoid are equivalent, because, 'about the same axis, and with the same radius of generation, they generate equivalent solids.' But the trapezoid is $\frac{5}{8}$ of the rectangle; therefore the parabola is $\frac{5}{8}$ of the rectangle."

The *functions* of a cone, cylinder and sphere are the radius of computation and the altitude of the body. In the case of the paraboloid, the *functions* are the parameter and abscissa of the parabola, of which the *latter* is the radius of computation; whilst the functions of the circumscribing cylinder are represented by the ordinate and abscissa of the parobola, of which the *former* is the radius of computation; showing, very evidently, that the paraboloid and cylinder are not generated *under the same conditions*. The circle being greater than any other isoperimetrical curvilinear surface, and the parabola *not being a figure of revolution,* it will require a less *generating* surface than the parabola to produce a volume of revolution equivalent to the paraboloid under the radius and altitude of the cylinder.

The parabola not being generated by revolution, the principles of revolution are not applicable to it; and the paraboloid is generated by a section of the parabola which does not preserve a constant equality of radii; hence, the conditions are not the *same* with any volume generated by a constant equality of radii as the sphere, cylinder and cone.

The functions of the paraboloid are derived from the properties of the parabola, in connection with the principle of revolution; but the properties of the parabola are so distinct from the properties of the circle, that the properties of the paraboloid have dissimilar conditions from those of the circumscribing cylinder, because it has been shown that the functions of the paraboloid and circumscribing cylinder are entirely distinct.

But, in the case of the volumes generated by the trapezoid B S N P, and the quadrant B N P, (BENSON'S GEOMETRY, p. 164,) *the conditions are the same;* hence, THE SAME CONCLUSION CAN NOT BE DERIVED BY REVOLVING SECTIONS OF THE PARABOLA AND CIRCLE.

In Prof. QUIMBY'S last letter, he wrote: "I would like to know how you get around the conclusion of the Differential Calculus, which gives the area of the circle the same as the old method by polygons. If the Calculus is wrong there, how do we know it is right on the parabola, &c.?"

Calculus treats problems in the same manner that they are treated in Elementary Geometry, which *is the system of rectilinear truths.* And

in order for the circle to be treated in Calculus, it had to be regarded as *a great number of short, straight lines.* Now, the straight line and curve differ essentially ; and the circle and polygon have no properties in common ; therefore, the treatment of the circle in Elementary Geometry and Calculus is fundamentally wrong. The fact that Calculus gives the same conclusion that Elementary Geometry does in the case of the circle, does not confirm the conclusion of Elementary Geometry ; because Calculus is based upon the principles of Elementary Geometry, and the same errors that have crept into the rudiments have permeated through the trunk and branches of geometrical science.

The parabola is not treated in Elementary Geometry ; but in Analytical Geometry, the properties of the parabola are confined to the condition that makes *the constant equality of two varying straight lines;* While the algebraic, transcendal and exponential equations of the parabola are derived from *the system of rectangular co-ordinates ;* and in no case are the properties of the parabola derived by treating its curve as *a great number of short, straight lines ;* hence, there is no conflict with any definition, axiom, demonstration or principle of Geometry ; consequently, the conclusions in regard to the parabola are legitimate.

Prof. William Chauvenet, LL. D., Washington University, St. Louis, lately deceased, wrote me : "I shall be willing to try to convince you of the fallacy of your conclusion in regard to the area of the circle, if you will answer, categorically and correctly, the following questions :

"Is or is not the area of a regular dodecagon inscribed in a circle, equal in area to three times the square on the radius of the circle ?

"Is or is not that dodecagon less than the circle ?

"These are questions which can be answered without recourse to the use of revolving surfaces, &c., and all the world can judge of the correctness of the answers."

It will be noticed that the questions of Prof. Chauvenet embody the objections made by "Dalton" to the conclusion of my first demonstration referred to in the beginning of this treatise ; and these objections, and my answers to them, were submitted to the committees to whom I have also referred. My reply to Prof. Chauvenet was the same in effect as my reply to "Dalton."

In geometrical demonstration, the straight line and curve are used : and although there are *many* varieties of the curve, there is but *one* kind of straight line ; because, according to the definition of the straight line, there can be but one kind *having the length always in the same direction :* whilst curves *do not have the length always in the same*

direction; consequently curves admit of innumerable changes. Hence, it is possible to give a definition embracing all straight lines, but impossible to give the innumerable individual characteristics of curves under one definition; and since these characteristics of curves are so distinct, a separate definition for each is required before curves can be successfully treated in the Elements of Geometry. In the absence of these individual definitions for curves, geometers have been forced to confine themselves to the properties of the straight line, and construct the science of Geometry upon those properties.

It is well known that *parallel straight lines* are a most powerful instrument in geometrical investigation; and they bear *prima facie* evidence that *curvilinear* magnitudes will not admit of their application; for it is a well known fact that *approximate* results only are obtained when the reasoning from *parallel straight lines* is applied to curvilinear magnitudes. Now, since the straight line is so different from the curve, conclusions derived from the properties of the former *must necessarily be* different from the conclusions derived from the properties of the latter; and the conclusions in the one case will appear antagonistic to the conclusions in the other case; but these conclusions, drawn from sources so opposite, will be perfectly consistent, logical and valid in *their own peculiar bearings.* Hence, a logical conclusion obtained from *certain* conditions must not be confounded with another logical conclusion obtained from *different* conditions; because, when we reason from the peculiar nature and formation of the circle, we show for the circle certain properties which *will be necessarily* incongruous with the properties of rectilinear magnitudes; and when we reason from *parallel straight lines,* we will show for rectilinear surfaces certain properties which will *approximate* only to curvilinear surfaces. Therefore, according to the principle of *parallel straight lines,* THREE TIMES SQUARE OF RADIUS will represent the area of a regular inscribed dodecagon; but, according to the nature and properties of the circle, THREE TIMES SQUARE OF RADIUS will represent the area of the circle. As paradoxical as it may appear, there is no conflict with the well known axiom *that a part is less than a whole;* because these conclusions are derived *independently* of one another, and they relate to *separate* and *distinct* conditions. The fact that the conclusion, *that the circle is three times square of radius,* agrees with *numerous* established truths relating to the cone, sphere and cylinder and the lune must outweigh its disagreement with *one* truth relating to polygons, when we take in consideration that there *is* a peculiar connection between the circle and the cone, sphere, cylinder and lune, and there is *no* peculiar connection between the circle and polygon.

Prof. CHAUVENET remained silent after my reply.

Several letters passed between Prof. Robert D. Allen, Kentucky Military Institute, Farmdale, Ky., and myself, preparatory to a series of discussions upon the various geometrical subjects embraced by my views, with the intention of having them put into pamphlet form for general circulation. But, for some reason or another, nothing further was done, although I was always ready to do my part. I respectfully invite Prof. Allen's attention to the points I make against the *Reductio ad Absurdum*, given in my reply to Prof. Quimby's defence of it, and I shall be glad to hear from Prof. Allen on the subject.

When I arrived in London, May, 1864, Prof. A. T. Bledsoe, LL. D., who was formerly Professor of Mathematics in the University of Virginia, sent me an invitation to call on him, which I did; and in course of conversation Prof. Bledsoe informed me that he had heard that I was about to publish a work on Geometry. I remarked to him that I have such a work in preparation. He then said, "If you will bring me the manuscript, I will examine it; and I will give you some advice about it; and I will be very frank and candid with you." I thanked him for his kind offer, and told him that I would bring him the manuscript on the next day. The following day I handed Prof. Bledsoe the manuscript; and after two or three days had elapsed, I called on Prof. Bledsoe to learn what he had to say. He said: "Young man, I advise you not to publish this work." I inquired "Why?" He responded, "Because you are wrong." I asked him, "Where am I wrong?" He replied, "You are wrong all over." I requested him to point out one place where I was wrong, and remarked: "I have a right to my opinion; if you think me wrong, the burthen of proof rests with you; and you should not denounce my work unless you can prove where I am wrong. I base my entire argument upon the single proposition *that the arc of a quadrant is* geometrically *equivalent in length to the sum of the chord and altitude of the segment of the quadrant;* and if you disprove it, I will acknowledge that you have refuted my argument, and I will not publish anything more on the subject." He hemmed and hawed, and finally said: "You do not know anything about Geometry; if you come to me, I will teach you Geometry in six months; but you must give up all you now know and begin afresh." I thanked him for his "frankness" and "candor," and remarked: "So long as my proposition remains unrefuted, I will hold on to my own views." He rejoined: "You have nothing new: I taught the same ten years ago, at the University of Virginia." I replied: "If you taught the same years ago, it is very strange for you to make the objections you do." He requested me not to discuss the subject any more with him, remarking that the discussion would make him think of nothing else, and dream all

night about it; therefore, in our future interviews, this subject was eschewed from our conversation.

A few years afterward, when I had published my Geometry, I received a letter from Prof. BLEDSOE, who was then editor of the Southern Review, in which he requested me to send him the names of those who approved of my Geometry, stating that he would make them "the subject of a long article in the pages of the Southern Review." I wrote him that I could not conceive in what way the names of the gentlemen who approved of my Geometry were concerned in the matter; that when it suited me I would publish their names in my own way. I invited his attention to my *arguments*, and requested that he should make *them* "the subject of a long article in the pages of the Southern Review."

A short time ago, I accidentally learned that Prof. BLEDSOE published, in the winter of 1868–69, a long article in the Southern Review, severely criticising the gentlemen approving my views, my Geometry, and myself.

I am surprised that Prof. BLEDSOE, who was once Professor of Mathematics in the University of Virginia; who is a doctor of laws; who is the author of some books; who is the editor-in-chief of the Southern Review; who was once a zealous laborer for the "Lost Cause;" and who filled some prominent positions under the Confederate Government, should publish such criticisms, circulate them, and not bring them to my notice. Such conduct appears to me very suspicious, and it resembles the assassin's trick—"*a stab in the dark.*"

ADDENDUM.

The attention of mathematicians is called to the following; it is also to be found in BENSON'S GEOMETRY, pp. 165, 166, 263, 264:

Circles are to one another as the squares described on their diameters (BENSON'S GEOMETRY, V, 14); consequently, squares are to one another as the circles described on their sides (BENSON'S GEOMETRY, V, 14, Cor. 2); hence, there is an equality of ratio between the circle and squares described about the same straight line; hence, there is the same *arithmetical* relation between the circle and the inscribed square that there is between the circumscribed square and circle. To prove this, let the diameter be 10; circumscribed square is 100;

inscribed square is 50; and let the circle be x. The arithmetical progression is 50, x, 100; whence $x - 50 = 100 - x$. The geometrical proportion is $100 : x :: 50 : \frac{1}{2}x$; whence $100 - x = 2\ (50 - \frac{1}{2}x)$.

Substituting for $100 - x$, its value from the arithmetical progression, we have, $x - 50 = 2\ (50 - \frac{1}{2}x) = 100 - x$. That is, there is an agreement between the arithmetical progression and the geometrical proportion. Now, the latter is unimpeachable; therefore, the former is also unimpeachable.

The principles used here are the obvious ones, viz.: *that the difference between two consecutive terms of an arithmetical progression is equal to the difference between the next two consecutive terms;* and *that the difference between the first and second terms of a geometrical proportion is as many times the difference between the third and fourth terms as is the ratio between the first and third terms.*

Hence, since $x - 50 = 100 - x$, we have $x = 75$. Or, THE CIRCLE IS THE MEAN AREA BETWEEN THE CIRCUMSCRIBED AND INSCRIBED SQUARES.

Hence, we have THREE distinct methods to prove the proposition THAT THE CIRCLE IS THE ARITHMETICAL MEAN BETWEEN THE CIRCUMSCRIBED AND INSCRIBED SQUARES, viz., by the Arc of the Quadrant, by the Revolution of Surfaces, and by Proportion,—most convincing evidences of its truth, even independently of the fact that the proposition agrees with *established* truths of Geometry relating to the Cone, Sphere, Cylinder and Lune. "FACTS ARE STUBBORN THINGS."

Very respectfully,

LAWRENCE S. BENSON.

NEW YORK CITY, October, 1871.

BENSON'S GEOMETRY

SIMPLIFIES THE SCIENCE,

EXCLUDES THE REDUCTIO AD ABSURDUM AND

PROVES THE METHOD OF APPROXIMATION

AN ERROR IN EXCESS.

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t is recommended by eminent Professors of mathematics,

ieties of Science and is adopted by the Board of Education

of New York and prominent Colleges and Schools.

NEW YORK.

BURNTON BROTHERS.

149 Grand Street.

LET TRUTH PREVAIL THOUGH THE HEAVENS SHOULD FALL.

CERTAIN publishers alarmed by the success attending the introduction of BENSON'S GEOMETRY into the prominent Colleges and Schools, have endeavoured by a series of adverse articles in some educational journals to detract from the merits of the work. They have siezed upon the difference between Cor 2. Prop. 17. Bk. 6. BENSON'S GEOMETRY. and the *Reductio ad absurdum* of Euclid and the Method of Approximation.

The articles which appeared in the American Educational Monthly, are based upon the law found in Weisbach's Mechanics (Vol. 1, p 106.) in regard to the revolution of surfaces around an axis. This law depends on the exact equality between the ratio, which we will call x, of the circumference to the diameter, and the factor, which we will call y, which multiplied by the square of radius gives the area of the circle. The above Monthly is unable to demonstrate the equality between x and y, but gives certain "processes" by which they are shown to differ, and adds "It is easy, by continuing the processes above, to prove a nearer equality of x to y" The tenability of the position assumed by the Monthly requires a positive proof of equality, and its subterfuge of "a nearer equality" evinces the weakness of its position. Mathematics is the exact science and the utility of its deductions demands exactness to give correctness. For this reason. CARNOT in his *Reflexions sur la Metaphysique du Calcul Infinitesimal* states that those processes were not considered by the ancient geometers as consistent with the strictness of geometrical reasoning, and PLAYFAIR protested against those processes as follows. "In this way. also, the circumference and the area of the circle may be found still nearer the truth, but neither by this, nor by any other method yet known to geometers can they be exactly determined, although the errors of both may be reduced to a less quantity than any that can be assigned," The method of reasoning pursued in BENSON'S GEOMETRY obviates *approximate* results and reduces the *errors* in the circumference and area of the circle to nothing. The American Journal of Mining takes exception to the same corollary. but its "argument" being a tirade of personal invectives and abuse, gives sufficient evidence of its own weakness.

The Author of BENSON'S GEOMETRY has published a demonstration whereby the Method of Approximation, and the *Reductio ad absurdum* reasoning of Euclid have been shown to make great *errors* in *excess* when applied to curvilinear spaces, consequently he has been in receipt of numerous communications from prominent mathematicians in various parts of the country and a few extracts from the letters of Prof. E. T. Quimby. of Dartmouth College. Hanover. New Hampshire. will be pertinent on this occasion. In his letter dated May 25th 1868, he states "I perfectly agree with you when you say that if the method of approximation gives an area for the parabola greater than $\frac{2}{3}$ the rectangle on the ordinate and abscissa, then either this method is wrong, or the demonstration of the

eorem giving this area to the parabola is fallacious." At the conclusion of this tter he writes: "I shall be glad to continue this correspondence till we get the ght of the matter if it be your pleasure, and I desire that you would very freely int out any errors I may make as I shall wish to do any I may discover or nk I discover in your work." Again, in his letter dated June 3rd 1868. writes I have been looking at your approximation computations of the area the parabola in which you get an excess above the $\frac{2}{3}$ rectangle. I have not ade the numerical computation because I presume you have been over them nes enough to secure accuracy. If you have made no mistake of this kind I ink the difficulty must be in not carrying out your work to a sufficient mber of decimals. Carry it out to 15 decimals, and see if your excess is not less" reply, it was shown that where more decimals are used, by addition, the sult becomes greater, and that if the work be carried out to 100 decimals, the cess would be greater than when 6 decimals only are used. In his letter dated ne 24th 1868, he proposed to let the error in excess in case of approximation "for the present" and turn his attention to the demonstration, proving the cle $\frac{3}{4}$ of the square of its diameter. In his letter dated July 16th 1868 he writes I proposed to lay aside the question of approximation for two reasons, 1, I had t time to make the calculations, and 2, I considered the other the main estion." But in his letter dated June 24th 1868, he gave the following propo- ion as an *Axiom*, "When two equivalent surfaces are moved in a line perpen- ular to their plane, or revolved, that one which moves farther generates the eater volume:" which is the same as TWO SURFACES WHICH GENERATE EQUIVA- NT SOLIDS WHEN REVOLVED ON THE SAME RADIUS, AND AROUND THE SAME AXIS E THEMSELVES EQUIVALENT. Upon the latter proposition the demonstration the above corollary in BENSON'S GEOMETRY is based.

But Prof. Quimby makes a particular application of his *Axiom* and in der to sustain his views, he asks assent to the following proposition in his letter ted July 8th 1868. viz "The volume generated by any plane figure revolved out a line in its own plane is measured by the area of the figure revolved, multi- ed by the mean circumference described by its points." This is in bstance the law quoted by the Am. Ed. Monthly from Weisbach's Mechanics and likewise depends upon the equality of x to y before mentioned.

is incumbent upon Prof. Quimby and the Amer. Edu Monthly, to prove the equal- of x to y, which Prof Quimby admits in his letter dated July 16th 1868, in the llowing words — "I admit what you claim that it is my business to prove it so. vill write you again at length on the subject" But in his letter dated Oct 7th 1868 excuses himself from giving the proof on the plea that his time is so much occu- ed and the proof would be so long that it would be an "infliction" To prove the

above equality; the Amer. Ed. Monthly however, uses the Method of Approximation, which has been proven to give errors in excess. when applied to curve surfaces, The ancient geometers, PLAYFAIR, TORELLI, and other learned modern mathematicians have protested against its use to determine the properties of the circle, and CARNOT in his *Reflexions sur la Metaphysique du Calcul Infinitesimal* after stating that the ancient geometers did not regard the Method of Approximation as perfectly rigorous nor consistent with the strictness of geometrical reasoning, adds that, the continual approximation of these polygons to the curve, afforded an idea of the properties of the latter, but it still remains to be proved by some recognised principle of demonstration the truth of the properties that had thus in a manner been divined. For this reason ARCHIMEDES proved the properties of the parabola from the relation that certain rectangular figures described about the parabola have to one another, and TORELLI, who was so well versed in the reasoning of the great Greek geometer, proved by the same process of reasoning that the circle has to the square on its diameter exactly the ratio of 3 to 4. PLAYFAIR who investigated Torelli's demonstration, admits that the proposition upon which the demonstration is based, is true on certain conditions. Which makes the demonstration perfectly valid, sound and true. because the fact of the proposition being true on any conditions whatever shows that it contains the essentials of a geometrical proposition since every proposition of geometry is conditionly true only; and a false proposition can not be true on any condition. Torelli's demonstration is therefore perfectly legitimate. and being the same process of reasoning as that used by Archimedes for the parabola, it is a recognised principle of demonstration. As a collateral evidence that Torelli's demonstration is true, the Method of Revolution another recognised principle of demonstration can be used. From Prof. Quimby's axiom in his letter dated June 24 th 1868. before given the conclusion derived by Torelli's demonstration is also obtained.

And Prof. Quimby in proposing to "drop" the subject of "the error in excess of approximation" and in evading the proof his proposition in his letter dated July 8. 1868. although he desired "to continue this correspondence 'till we get the right of the matter," evidently found that the law quoted from Weisbach's Mechanics is undemonstrable and untenable, and is consequently indefensible, null and void.

MY VISIT TO THE SUN;

Or Critical Essays on Physics, Metaphysics and Ethics,

By Prof. LAWRENCE S. BENSON,

Author of BENSON'S GEOMETRY.

Revised and Enlarged from the English Edition.

IN PREPARATION FOR THE PRESS.

Prof. G. B. Docharty, LL. D., of the College of the City of New York, writes:

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